Chapter X

CONCLUDING REMARKS

A REVIEW OF PERSPECTIVES

The preceding chapters in this volume make it clear that generalization about ARPA is especially difficult, if not impossible. The Agency simply cannot be tied up in a neat conclusionary package that purports to represent the essence of its seventeen years of operation. Indeed there appear to have been several ARPA's, each reflecting rather accurately the tensions and/or opportunities of the environment within which it was set.

At most, about one-third of ARPA's lifetime can be said to have been "normal." Much of the time it functioned in the midst of considerable bureaucratic stress. Outright abolition was widely discussed in 1959 and apparently quite seriously considered at the Secretary's level a decade later.

ARPA's tribulations have often been rather reliable indicators of forces and trends external to the Agency, although frequently those working within ARPA did not sense that fully at the time. The ebbs and flows of ARPA budgets, for example, reflected very clearly the ebbs and flows of DOD and RDT&E budgets generally. The relative rise and fall of the scientific elite and of the faith in science and technology's power to resolve all major national policy issues definitely affected perceptions of ARPA and what it was supposed to be doing. The evolution of the Office of the Secretary of Defense, especially ODDR&E, conditioned the uses to which ARPA was put. Many political and policy debates in Congress and the Executive Branch, e.g., ballistic missile defense systems deployment, nuclear testing and arms control policies, and the role of the United States in Asia, directly affected the Agency's programs.

During its first decade, ARPA's leadership tended to feel that the Agency was a unique organization in DOD with special ties to the Secretary and hence somehow immune from the impact of many of the forces and decisions that shape the activities of the Services and other parts of the Department. The assumption concerning organizational uniqueness was correct, but the derivation of immunity was not. ARPA was far more an integral element of the Defense Department than it cared to admit and this lesson was brought home rather severely in the post-1967 period.

There is no doubt that as the period under review in this study ended, ARPA was a much more constrained agency than earlier models. In fact, since 1967 it has been consistently receiving and even asking for less money, year by year. Figure X-1 illustrates this point, showing the downward slope of ARPA budget requests calculated in constant (1967) dollars. In the first ten years of ARPA's lifetime, the average Congressional cut in ARPA's budget

FIGURE X-1

TREND IN ARPA BUDGET REQUESTS
Constant 1967 Prices

(S millions)

SPACE
PROGRAMS
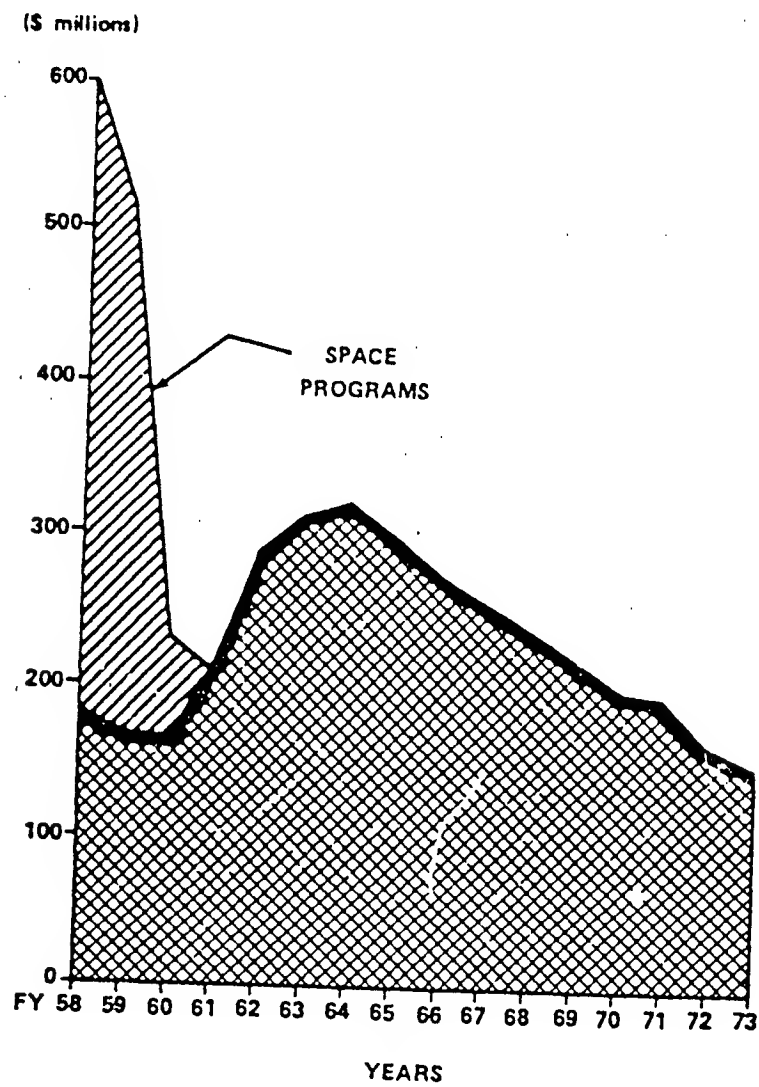
YEARS

requests was $2.6 million, the highest cut was $7.0 million and in five of those years there was no cut at all. By contrast, in the period FY 1969 through FY 1975, the average cut in ARPA's budget requests has averaged $18.9 million and the lowest of these annual Congressional cuts was $11.9 million. Since FY 1969 the ARPA program budget has averaged $205 million per year.* Obviously ARPA has been cut back (as have many other DOD RDT&E agencies and programs). A number of respondents' observations sprinkled throughout the remainder of this chapter implicitly, if not explicitly, are conditioned by this fact. A number of them speak about ARPA in the context of an agency with considerably greater funding levels than the Agency in fact now enjoys and with a "margin of flexibility" no longer available in the form of unobligated balances, forward funding options, etc.

Changes in ARPA's status over time are not particularly surprising, but we have been struck by the relative lack of discussion or debate either in the Secretary's office or the DDR&E's office about how to use an agency like ARPA, other than the essentially reactive attention that it has received when it was or appeared to be in trouble. Aside from the McElroy period, successive Secretaries have not paid much attention to it. Even the DDR&E's have seemed to be faced with a wide range of problems far removed from ARPA. They were unable or saw no need to give ARPA much time, especially Drs. York and Brown. Dr. Foster did for awhile, but the main reasons seemed to be a felt bureaucratic need to bring the Agency under proper management control and to use it to meet Vietnam emergencies. This absence of attention may correctly reflect the declining importance of crucial national security issues defined in R&D terms. At the least, the Sputnik era propensity to define issues almost entirely in terms of science and technology is clearly dead.

One may ask why Secretaries or the DDR&E's have not abolished the Agency. Aside from the fact that ARPA has done a number of undeniably useful things, a principal reason is probably its budget. If he so desires, a Secretary or a DDR&E can rather strongly influence the use of that money; it permits them, in theory, to handle things that "fall through the cracks;" and it may help them to deal quickly with problems that come up overnight, when there is not time to go up to Congress for authority and money. The phrase "may help" is used advisedly because, at present, as Rechtin observes, "ARPA's original charter and ability to move quickly, and take major amounts of money and throw it into something like [an emergency] -- and not have everybody stand around and argue about it -- is heavily diluted right now."[1] DDR&E's also have had a much easier time stopping Service programs than they do in getting the Services to start and sustain properly something that they do not want to do. York, for instance, found ARPA a great convenience for starting the Materials Sciences program. The Services might have done it, but ARPA did it: "There is just less argument with ARPA about getting something like that going, and doing it, than there is with the Services."[2]

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* The most recent ARPA funding history, from which these data are derived, is reproduced as Appendix A at the end of this chapter.

Foster drew on that characteristic even more intensively, though usually for somewhat shorter range purposes. Note though that the usual DDR&E outlook on ARPA is conservative, restrained, restrictive. It is basically ARPA as a convenience rather than a necessity. This outlook relates to the discussion below of planning how best to use an ARPA.

The ARPA Role

A search for the essence of the ARPA role is an elusive one -- its charters have been both imprecise and comprised of multiple components. This has long been a source of continued "why ARPA?" questioning and a source of strength, in that it is impossible to pin down one single jus-tification for the Agency that either makes it obviously indispensable or, conversely, permits it to be categorized as obsolete. As is clear from the preceding pages, ARPA has performed work of major interest to no Serv-ice, to one Service and to several Services. It has undertaken both very "high-risk" projects and institutional support programs for which the very concept of risk appears inappropriate. It has served OSD and the DDR&E very directly, supported work which OSD/DDR&E barely tolerated, and under-taken projects which interested no one outside of ARPA and the project performers. It has worked on issues of grand national policy debate, prob-lems of interest primarily to some specific Service component, and projects incomprehensible outside of a technical laboratory. Its portfolio covers a wide range of both military concerns and scientific disciplines.

If pressed to reach a "definitive" definition of the ARPA role, we would be tempted to state that ARPA is the agency which, in principle, undertakes programs that: (1) have at least one of the characteristics usually attributed to ARPA work, namely, "high-risk," multi-Service, falls between Services, high OSD interest in central management, etc., (2) have no alternative home in the contemporary R&D bureaucracy, whether due to roles and missions disputes, budgetary constraints, administrative compli-cations, or lack of interest, and (3) have some promise of more than marg-inal significance either within a technical field or in terms of Defense implications, or at least have a persuasive advocate to that effect. That is, an ARPA program is typically a product of a positive rationale for its assignment, a negative rationale for its not being assigned elsewhere (ARPA as a convenience), and a level of special interest in the substance of the program. All three characteristics are essential. A "high-risk," multi-Service interest program can be, and often is, undertaken on a joint Service basis and the Services normally can make a strong claim for this approach. Therefore, for ARPA to receive a "multi-Service" assignment there must be at least some transitory negative reason for the DDR&E not to choose this option, either due to administrative inconvenience or to policy reserva-tions. Since ARPA does not have a charter to support routine R&D on a continuing basis, there is also normally a requirement to establish that there is something unusual or important about the prospective work (though the definition and threshhold of what is deemed important may vary considerably).

While the above may better describe the ARPA program than simply declaring ARPA to be the DOD's "anti-surprise" agency, it is obvious that it leaves room for considerable programmatic heterogeneity and varying emphasis. Not surprisingly, therefore, the numerous respondents who contributed to developing this history assessed the ARPA role from many different perspectives. Following below is a review of some of those perspectives, including ARPA's role in responding to major technological opportunities, filling gaps in Service R&D, providing flexibility and quick reaction capabilities, relating to "Presidential Issues," avoiding "technological surprise," and supporting basic research.

Responding to Technological Opportunities. Virtually none of the senior persons with whom we talked is prepared to say that without ARPA, various scientific and technological achievements would not have occurred. Such black and white views are foreign to the scientific temperament. Respondents, rather, commonly say that ARPA may have expedited the appearance of a particular technique, device or technology -- perhaps by several years -- but that one way or another most of the ideas supported by ARPA (or any other technical agency) would have come along anyhow:[3]

> [If you ask] how does it compare with other vaguely comparable elements of the Defense Department, in other words, how did ARPA manage defense R&D compared to how the Air Force or the Army or the Navy manages defense R&D, I think the answer is they [ARPA] do fairly well. On the other hand, if they had been eliminated, if they had never been invented, would the United States' defenses be weaker? I think probably not. Would they have cost more or less? Darned if I know. But it was a competent group who did their job reasonably well and who made some things easier for the other people in OSD.... I don't know whether [ARPA had] a better batting average [than other agencies] or not. I think ARPA has done fairly well in the past. I don't know whether ARPA still does well or not. I'm really not that close to it.... I think then [early 1960's] it was pretty good, but other people's batting averages were not zero. They [other agencies] might not be terribly high, but they weren't zero. There were good and bad groups everywhere.

Sproull remains convinced that without ARPA there would not have been a Limited Nuclear Test Ban Treaty, but other than that he tends to second the point of view expressed above:[4]

> I don't think you can say anywhere that 'without ARPA you wouldn't have had this,' but I think you can say that the ABRES program was a much more

> effective program because of ARPA, in that the
> Army ballistic missile Defense program [agency],
> ABMDA, was a much more effective organization and
> [made] much better use of federal funds because
> of the existence of Project DEFENDER, and because
> of the continued goosing and sort of technological
> audit and 'keeping honest' role of ARPA.

The simple act of expediting good ideas, of course, can be exceedingly
cost effective, e.g., those who argue that without DEFENDER's success in
highlighting flaws in ZEUS and suggesting an alternative approach, the
ZEUS system would have been built at immense cost. Moreover, the cost
effectiveness of accelerating such fields as computer time-sharing and
networking or various areas in materials science may have been enormously
significant, given the extremely broad potential influence of developments
in these fields to the DOD and the civilian sector of the economy. This
suggests that ARPA's role may be less one of supporting pure discovery or
innovation and more that of choosing selectively from a market basket of
ideas certain preferred items for accelerated development.

Dr. York, who is not one to argue that there must be an ARPA or that
ARPA stood head and shoulders above other R&D agencies in terms of per-
formance, agrees that it was useful and that it was characterized particu-
larly by an ability to recognize good ideas.[5]

> The question of 'was there a pay-off?' can be
> asked at so many levels.... One level is that in
> a country like this with several million people
> working on defense questions and defense R&D (maybe
> a million persons, or half a million, I guess) it's
> just hard to imagine that if you take out any par-
> ticular individual or any particular group of
> individuals that those same ideas don't just arrive
> somewhere else anyhow ... and probably not terribly
> long after.... There, though, the question is whether
> there is a management group that's listening and
> that can understand what it is being told; that's
> probably more important than where the idea comes
> from ... and there I think of ARPA, as being part
> of management, rather than originating. It's
> probably the ability of ARPA to listen to ideas,
> and to buy good ideas, that's more important than
> their ability to think up good ideas.

General Young, whom it will be recalled supported dissolution of ARPA in
late 1959, believes that the Agency was excellent at selecting ideas and
developing a sensible program with regard to outer space in the midst of
the post-Sputnik chaos. Many space project ideas were floating around in

1958. What ARPA did was to structure or order incipient programs. As he said:[6] "ARPA picked the proper things to do, put the right amount of money on them, shaped them into coherent programs, and eventually turned them over." His characterization of the Roy Johnson ARPA has in fact been the ideal of most of Johnson's successors.

Dr. Sproull is very keen on this function of recognizing good ideas. He believes that if an agency like ARPA is properly handled:[7]

> [I]t will keep a lightfootedness that will enable [the Department it is in] essentially to continue to modernize itself, which a federal agency has a terrible time doing. [Having an ARPA enables a Department] to get a conduit for new ideas that may be unpopular ideas, challenging ideas, uncomfortable ideas, and contact with a world of ideas and people who may not be all that accessible to the standard part of the [Department].

As of the end of the period under investigation in this study Lukasik characterized all ARPA staff members as "sponges," soaking up ideas from everyone, rarely limiting themselves to a restricted number of sources.[8] The "high-risk" quality attributed to ARPA often has meant little more than a better than average willingness and ability to recognize and support ideas from unlikely sources that otherwise might not get through the "peer review" committees at the NSF or the layers of review boards and groups used in many R&D organizations, in the Services and elsewhere.

This ability to recognize good ideas ties in with York's view of ARPA as playing an important role in the "idea exchange mechanism" at high levels within the DOD and between DOD and the outside world. For many years ARPA was willing to support new or high risk areas of research and to make the results widely available to those working in R&D. This, according to one of our respondents, is the only way to advance the "technological culture." [9] His model was the advanced BMD technical community that ARPA supported and stimulated so successfully. Even though much of that work was classified, ARPA went to great lengths via paper circulation, information centers, symposia, special study groups (with broad participation from university, government, not-for-profit, and industrial people), supporting alternative approaches to technical problems, and other devices to make the data available and to solicit debate.* The combination of ARPA's very low-profile

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* This was true even for the highly sensitive penetration aids work, e.g., ARPA arranged for relatively wide circulation of its compilation of experimental reentry vehicle performance data specifically to encourage a "self-cleaning" system, that is, one in which there were incentives such that corrections, modifications and up-dating would be almost automatically forthcoming from the most knowledgeable participants.

policy since 1968-1969 and its greater involvement in intelligence related
R&D in the 1970's has perhaps reduced this characteristic somewhat, but
ARPA is still generally regarded as superior to other agencies in arranging
✓ the exchange of information and ideas within communities of interested
scientists and technologists. The absence of central program themes in
ARPA and the recent tendency to support relatively small, discrete projects
and program elements, however, might be expected to reduce this feature of
ARPA's style unless these projects come to coalesce around a few major
disciplines, fields or missions.

If <u>recognition of good ideas is a reasonably continuous hallmark of</u>
<u>ARPA's performance</u> over the years, it follows, as many contend, that "good
people" -- staff, consultants and contractors -- are the key to the Agency.
This is a truism valid for any organization, but many take it to be the
distinguishing characteristic of ARPA.

Every ARPA Director has unusually strong, positive words to say about
the quality of the ARPA staff he directed. We have also found, to almost
the 100 per cent level, that every ARPA professional considers his tenure
at ARPA <u>the</u> highlight or one of the highlights of his career. This in-
cludes some who left angry, or quit because they disliked a Director or
office chief, or resigned over the Vietnam War or other policy differences,
or who departed because of differences over program content or direction.
It is little short of amazing to find such consensus, expressed in such a
forceful manner.

The common denominator running through the opinions of both the happy
and the disgruntled appears to be that each was able to immerse himself in
the substance of some work that he thought was significant and interesting,
feel that he had played a definite role in shaping it or saving it from its
detractors, and believe that the outcome of his efforts would be of above-
average value to a scientific field and/or to the DOD. To illustrate this
point, we cite at length an ARPA program manager who lived through <u>the</u>
<u>Agency's most difficult days in the late 1960's and early 1970's</u>, as he
assesses his role and the Agency's role:[10]

> ARPA is growing older. There's no doubt about
> it. There's more red tape, more bureaucracy and
> less autonomy and less rapidity of response than
> there used to be.... [I]t's grown since I've come
> on board.... It's probably an unavoidable thing.
> Whether or not it's a good thing depends on the
> quality of people that you have. And I don't know
> whether ARPA's been lucky or whether there's some-
> thing built into it that causes good people to be
> here. I think its self-perpetuating to an extent.
> A place that has good people can handle the degree
> of autonomy which ARPA has, which is still enormous.

The power and authority of my position within
[my field] is quite significant. <u>I sit here behind
a desk and make decisions about millions of dollars
a year, without any review boards, or advice or pan-
els, or anything else that I don't specifically,
personally, want or request.</u> Now, I have to sell
that through ARPA, but I've had really very good
success in doing that.... With that amount of money,
which would be nothing in physics, you couldn't buy
a decent laser ... but in [my field] that has an
impact. I have made a significant impact in several
areas....

[I]f I were really dumb or dishonest or something
that I hope I'm not, it would be too bad. Then you
could point at it and say: 'How insane ... obviously
someone forgot to put in a review board.' Now that
would make everything take a lot longer. It would
be a lot less exciting work because you have to
please everyone ... or at least satisfy them, and
you would knock off a lot of the interesting parts
of whatever it is [you're proposing]....

[I]t seems to me there ought to be a place [with
ARPA's freedom] ... whether it should be in the De-
partment of Defense I don't know. I tend to think
not -- but on the other hand, if it's not buried
[could it survive?].... [Moreover this office] is a
tiny part of ARPA. Many people are just unaware of
its existence. And if it wasn't like that, if it
stood out there in the glare ... maybe it couldn't
exist. It would be so thoroughly scrutinized and
harrassed....

I was talking then about our office specifically,
but ARPA as a whole probably has an important place
within DOD. The Services certainly aren't going to
stick their necks out.... [T]hey attract people who
are conservative and cautious and slow and concerned
more with bureaucratic process than with substantive
research.... It seems to me that you badly need an
infusion of a little more life and activity. <u>ARPA
sparks things</u>, you know [some personal examples here
of pushing the Services to do things].... They [the
Services] would be taking a terrible chance to take
their money and put it into that kind of basic re-
search. If it didn't work they'd be in trouble; if
my stuff doesn't work, nobody really expects it to
anyway and nobody notices too much if it doesn't.
If nothing worked, Lukasik would have dumped me long
ago I suppose. But, you know, some of it works and

it's all interesting. And if it doesn't work then
it's good to know that it doesn't work.... So, it
seems to me that it serves an important purpose....
I think in general ARPA's ability to respond quickly
and take risks is an important thing and I don't see
how this could exist in any other agency. [In the
Services, any such budget] would just be sucked
away. You would never accomplish anything....
    [The Service R&D people and ARPA's Service
agents] are from my experience, well, dumb. That's
what it comes down to; not necessarily low IQ, but
not too much imagination or spark or aggressiveness.
They pick out these jobs, they spend their life in
them -- what the hell do you expect? They're bureau-
crats. That's what they are. They're more concerned
with ... the bureaucratic aspects: whether they can
get approval, whether it's going to make their office
look good, how it's going to affect the way their
budget looks to Congress. I don't give a [damn]
about that. It has to be attended to ... [but]
I want to think up research and have a fine sci-
entist bring my fantasies into reality. That's
what's fun.

Thus despite tendencies toward institutional rigidity ARPA apparently
retains a spiritual heritage tracing in part to the space era and in part
to the Ruina renaissance. It is a heritage that persists in maintaining
that ARPA is not routine, but rather concerned about the important, the
interesting and the innovative. Given the constraints on the Agency in
recent years, the fact that some ARPA personnel still feel oriented toward
bringing "fantasies into reality" and toward the "fun" of substantive tech-
nical accomplishment is probably remarkable. Lukasik paid tribute to the
quality of ARPA's people. Acknowledging that the Agency was theoretically
vulnerable to too much cronyism, incest, small in-groups, "shoveling out
the money," etc., he said that ARPA could be a gigantic scandal, but it
never has succumbed because of its "good people."[11]

This feature also helps to explain ARPA's apparent success in finding
and attracting high quality outsiders to serve as consultants, advisors,
members of study groups, contractors, etc. And good people have been
attracted to ARPA, both to work within it or associate with it, because
compared to the Services and others, ARPA lacked vested interests. It
tended to call the shots as it saw them, and it was recognized that as
part of OSD, ARPA often was close to the sources of power and decision-
making in the Pentagon. Its obvious role in DEFENDER, VELA and even AGILE
for a time, confirmed its status. ARPA could, more often than the Services,
gain access to talented people who would not otherwise take an interest in
national security issues.

ARPA's 6.1 research also played a special role in enabling it to press top flight people into service when needed in DOD:[12]

> One of the great strengths [of ARPA's 6.1 research]
> was that we were in first name, trust-one-another,
> contact with people throughout the country ... very
> strong people in any field that had any real [con-
> nection] with the Defense Department.... We had
> access at the end of the telephone to the best
> wisdom, analyses, advice, criticism in the country.
> Part of this was that mutual trust that built up ...
> that we always learned something from somebody coming
> into the office.

ARPA developed a "flair to interact back and forth" and to "get the most out of people as well as projects" that Sproull, for one, considered one of ARPA's most important attributes.[13] It was still in place, and just as highly valued, when Rechtin took over:[14]

> I thought that ARPA's contact with the technical
> community was a major asset. That was one that
> was there long before I got there. I tried to
> retain [it]. I think it has been retained since.
> It's a hallmark of ARPA, and that community is what
> generates the ideas.

There is another side to the people issue. Many observers choose to explain ARPA's troubles in the very late 1960's and afterwards largely in terms of a reduced ability to attract high quality staff. The reasons cited are obvious: unhappiness with Vietnam policy; the particular dis-enchantment of university people with the Defense Department and a variety of national security policies; the alleged anti-science bias in government symbolized by President Nixon's gradual dismantling of the White House science apparatus; re-emergence of a significant gap in government and indus-try salaries at the highest grades; increased unemployment, which makes those with a Civil Service appointment less eager to leave and those on the outside more sensitive to retaining their positions in industry or the universities; the attacks on ARPA's existence; the Agency's low-profile posture; diminution in funding flexibility and ability to free lance; a perceived decrease in access to the Secretary or involvement in major policy issues, etc. ARPA still attracts the outstanding individual for specific projects, it is felt, but not across the board. Dr. Ruina, among many others, believes that the quality of people in government generally has "gone way down."[15] ARPA is not immune to these trends and dealing with them is expected to be one of its major challenges for the future.

Filling Service Gaps. There is still a frequently expressed opinion that an ARPA is needed to handle projects of interest to the Secretary which a Service might ignore, to manage projects involving more than one

Service, to insure that alternatives to particular Service programs are
actively pursued, and/or to challenge the Services. It is sometimes
difficult to know whether this is a reflex, by-rote response or is actually
firmly believed. Rechtin and Lukasik routinely talked about working on
projects of interest to more than one Service, but because of funding re-
strictions and their emphasis on transfer, ARPA tends to do less of that
now on an exclusive basis and seeks to make its influence felt by partic-
ipating with the Services on programs of mutual interest. In the case of
lasers, Foster provided generous funding to the three Services and ARPA, a
reversal of the original 1958-1959 assumption that a Secretary or DDR&E
would usually be inclined to avoid multiple efforts. Programs of interest
to none of the Services were less likely to be picked up in the ARPA of the
early 1970's because the Secretary and DDR&E were no longer making such
assignments and ARPA as a matter of policy was tending to avoid initiating,
on its own, programs for which there was no clearly visible Service user.
Whether this condition is temporary or permanent is impossible to determine.

There seems to be a continuing consensus that ARPA can be very produc-
tive in stimulating or challenging the Services to do better R&D, but absent
solid support and protection by the Secretary and the DDR&E, "stimulation"
and "challenge" can quickly yield a backlash in which ARPA is criticized
for interference, wasting funds, creating confusion, or worse. While
Foster was sensitive to the latter criticisms, he nevertheless insists that
ARPA exists as "a mechanism to instill a new range of scientific and tech-
nological potentials" in the Defense Department and that he used it to do
things that "the Services wouldn't, couldn't or shouldn't do."[16] He
still asserts that ARPA's primary function is to excel in doing advanced
research; conversely the Services cannot be expected to excel in that role
because they have so many other functions to perform. For him, "science and
technology can have very high leverage on military systems and therefore it
makes sense to concentrate on the leading edge," and that is ARPA's purpose.
[17] The words could have been uttered by Roy Johnson.

Rechtin would underscore this viewpoint by arguing that in times of
budget stringency, the Services either do not or cannot protect their
budgets for advanced research. The great difficulty one has in weighing
this "leading edge" theory is that during the Foster/Rechtin/Lukasik era
ARPA seemed to be moving away from the leading edge as often as it approached it
Low profile, quick-transfer oriented, "no screwball ideas," obvious military-
relevance agencies are not well suited to the achievement of revolutionary
breakthroughs or major developments. Moreover, as Frosch suggests, ARPA
has not been especially successful in protecting its own budget for advanced
research.[18] A case in point is comparison of the original ARPA role in
ballistic missile defense R&D and its 1970's entry into antisubmarine war-
fare R&D. ARPA's ASW work is much more intimately linked to Service pro-
grams than were large parts of DEFENDER. The Agency has not been able to
get either a charter to match the open-ended DEFENDER assignment nor a
DEFENDER-scale budget. What Frosch calls "laboratory-scale financing"[19]
will not suffice, normally, to make major advances. It is well to remember

that the DEFENDER charter was virtually unlimited. As Sproull summed it up: "we did pretty well what we wanted," and the program had funding to match.[20]

In other fields as well, ARPA's ability to produce a "critical mass" of support for a given area of work often appears to be the dominant factor behind its success, and without it Service relevance may mean little. Thus one of the most pressing questions for the post-1973 tightly budget-constrained ARPA is whether it retains the ability to reach a threshold level of support that can really "make a difference" in a given technical area. Perhaps under the influence of stricter relevance requirements, and definitely reflecting the pressures of the Vietnam War, the "leading edge" in recent years increasingly has come to mean, in the view of several observers, some type of high technology "gadget" like lasers, NITE GAZELLE or the arctic surface effects vehicle. Indeed to some, ARPA has come close to being an "advanced procurement agency" rather than an advanced research agency, with occasional excursions into 6.3 (advanced development) and 6.4 (engineering development) territory.

Flexibility and Quick Reaction Capabilities. ARPA has always laid claim to being a flexible, quick-reaction agency. Initially, that meant being quick to recognize good ideas and to start them with a minimum of red tape. It did not necessarily mean quick results. Over time this feature of ARPA has been interpreted on occasion to mean "quick to get a result," e.g., the field test of the AR-15 rifle in Vietnam. The subsequent Vietnam ZAP channel work was based, pure and simple, on ARPA's ability to commit funds fast and the DDR&E's power to mandate, directly, that the necessary internal reprogramming be done to free up the funds. Indeed Dr. Foster describes ARPA as "a crisis agency,"[21] but the crisis response sought for Vietnam was considerably different from the Sputnik crisis response that stimulated McElroy to establish ARPA.

Prior to 1968, ARPA tended to think more about technologies than end items. It continued to be motivated, in fantasy if not in fact, by the Roy Johnson mission of serving as the Secretary's personal agency in R&D -- quick, agile and efficient in the task of giving the Secretary an independent "technological audit" in fields of special concern to him. This conception depends vitally on having Secretaries who think in such terms (an issue discussed further below), but accepting that assumption for the moment, this was a rationale for ARPA in the mid-1960's and one which Dr. Sproull, for instance, would claim is valid today:[22]

> One can move fast, can be responsive, one can be an
> extension of the arm of the Secretary if he wants
> to make sure that the Army program in something or
> other is moving as fast as it possibly can, and he
> is uncomfortable about that, he can be talking with
> ARPA people and putting some additional money in

ARPA, he can get kind of a technological audit of
that ... which would be more efficient than simply
putting more money into the first agency ... which
would have a tendency just to keep working at the
same ideas. I think it's a very good use of a
fraction of the money.

Lukasik's view of the need for an ARPA to avoid "technology perception
gaps" was very close to this perspective.

The DDR&E's use of ARPA's quickness in the late 1960's was less to
perform an audit for the Secretary than to respond to the enormous pressures
on his office for R&D contributions to the Vietnam War. Lukasik uses the
phrase "the urgent drives out the important" to describe this period, but
the phrase reflects a persistent, built-in dilemma for ARPA.[23] The
Agency's virtues as a convenience to the DDR&E simply are not always mutu-
ally supportive of other Agency missions. The Congress, for its part,
seems to prefer that ARPA function in the relatively large area that falls
between an Arecibo project on the one hand and buying hardware for immediate
field combat testing on the other. ARPA remains vulnerable to unduly nar-
row interpretations of its quick-reaction attribute.

"Presidential Issues." The most obvious dividing line between the
ARPA Directors concerns feelings about Presidential issues and ARPA's role
vis-a-vis the Secretary. These differences are well-illustrated by their
reactions to the hypothesis that ARPA has served its purpose, but now no
longer is needed, in part because there are no advanced "breakthrough" ideas
equal in significance to the missile/space/nuclear energy combination of
the late 1950's to work on and/or because the Secretary and the DDR&E can
now control Service rivalries and assign them work in full confidence that
it will be done. In other words, perhaps events have passed ARPA by.

Surprisingly perhaps, Dr. Killian rejects this notion:[24]

I now feel that ARPA is a useful agency in the
Office of the Secretary of Defense but the DDR&E
should be heavily involved in the decisions in re-
gard to its program. I do not think that develop-
ments in the military services or in their roles
and missions indicate reason to give up ARPA.

Dr. Ruina reacted to the same proposition with the comment: "That sounds
not unreasonable."[25] He added the caveat that if a Presidential issue
were at hand, he would want an ARPA, but he sees none (i.e., something that
might significantly alter the strategic weapons situation) presently on the
horizon. Presidential issues in which Service vested interests are involved
warrant an ARPA: "[I]n VELA and BMD an ARPA was absolutely essential."[26]
Without such issues, an ARPA is questionable. This leaves unsolved the

proolem of sustaining an ARPA-like capacity "between issues" or attempting to create a new one whenever an appropriate issue arises.

Drs. Sproull and Herzfeld also fall clearly in the Presidential issues camp, but are inclined to believe that there will always be such issues, or something akin to them, facing the Secretary. Herzfeld, for instance, believes that "any technical enterprise over one billion dollars needs a high quality, quick reaction shop" to handle that ten per cent of the problems that the organization's "steady state management" system will not be able to handle.[27] Dr. Heilmeier has also likened ARPA to a corporate R&D activity.* This choice of analogy is somewhat similar to the McElroy-Johnson "blue sky" unit.

Herzfeld's agency, however, is presumed to have very close links to the top, i.e., in DOD, a close "coupling" to the ecretary and his problems as the dominant customer. VELA and DEFENDER are his models. For these issues:[28]

> ... it was a good thing for the OSD, on program[s] this important, to have its own capabilities. And I have come to be convinced that's tne right answer. So much so that I think any very large organization really ought to have an ARPA in the office of the president. Because, if the stakes are high, it [the work] has got to be right, and it has to move as rapidly as possible.

Sproull's "technological audit" function is based on the notion that it makes sense for the Secretary and his DDR&E to devote some two to five per cent of the Department's RDT&E funds to that purpose, and that having an ARPA "is an effective and efficient way of doing it."[29]

The harsh reality of the Rechtin-Lukasik period was, as previously discussed, that the Secretary and his peers had largely lost interest in ARPA, apparently did not see it as especially useful for resolving that "ten per cent of difficult problems," and did not accept the need for an ARPA-directed technological audit on their behalf. One can speculate that during the McNamara era the growing conviction that new technologies were driving the arms race helped to create negative feelings about the advanced research agency. For instance:[30]

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* Heilmeier's ideal corporate R&D agency, and ARPA, are intended to serve five functions: (1) filling gaps, by covering high priority problems that cross Service lines, (2) doing unique or unconventional jobs, (3) tackling "the revolutionary rather than the evolutionary," (4) undertaking "high risk alternatives which may run counter to the conventional way of doing business," and (5) behaving as an agency with no vested interest in the status quo. (House Committee on Armed Services, Military Posture, Hearings, 94th Cong., 1st Sess., "Research and Development Subcommittee Title II, H.R. 6674," Part 4, March 24, 1975, 4903-4904.)

> Secretary McNamara and other senior civilian
> policymakers in the period after 1960 appeared to
> feel that any radical new innovation in weapons
> systems on the part of the U. S. defense establish-
> ment might destabilize what they hoped would be a
> relatively static kind of symmetry in the major
> weaponry maintained by the world's most militarily
> formidable nations. In this sense, then, there
> appeared to be conscious decision after 1960 not
> to allow the armed forces to push ahead on inno-
> vative ideas conceivably leading to major new
> departures in militarily useful technology.

ARPA, in its headiest moments, liked to think that it was chartered to do
just that, i.e., to "push ahead." One could even argue that York and Brown,
in concluding that there were many fewer exciting technical ideas with
breakthrough potential to work on, were permitting policy desires to color
their professional judgments. Such speculation is pointless, however, be-
cause the more "hard line" Nixon Administrations seemed no more interested
in ARPA than the Johnson Administration. They were intent on "tilting"
back toward the Services within DOD. Secretaries no longer assigned work
to ARPA. Moreover Rechtin confirmed the paucity of good scientific ideas
during his tenure. Thus ARPA Directors after 1967 could not operate on the
assumption that they were in fact the Secretary's agent.

ARPA staff during at least the first ten years of the Agency's exis-
tence identified very closely with the Office of the Secretary. This OSD
connection was a matter of great pride with most of them. Their OSD iden-
tity, if anything, transcended their ARPA identity in importance. For an
independent, flexible sort of agency, ARPA was markedly free of unauthorized
flights in directions purely of its own choosing, in the post-Roy Johnson
period.* The various Secretaries of Defense and their key associates probably
never realized the depth of this type of OSD loyalty and certainly failed to
exploit it as fully as they might have. Part of the tremendous readjust-
ment required when the "Presidential issues" programs were transferred or
closed out was a function of the feeling of loss by ARPA staff of close
connection with the Secretary and his concerns, which they had assumed more
or less as a birthright.

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* For instance, Project AGILE was created in part because Godel believed
  DOD needed to grapple with the insurgency problem, knew the Services
  would not, and felt that the Secretary would be forced by events to take
  the lead. Skeptical ARPA Directors tolerated AGILE because they believed
  that "somebody" had to do it and, at least initially, the signals from
  the Secretary's office were that it was important to him and to the
  President.

The challenge to the ARPA of the future may be in defining and maintain-
ing a non-Presidential issues role that is sufficiently unique and valu-
able to warrant retaining the organization. This problem appears to be
a real one:[31]

> [W]e now have an ARPA ... with some 40 projects.
> All of them quite good ... that is, the technical
> quality is very good. The transfer problem is
> licked; it works very well into the Services. In
> fact, on some projects you can't tell whether it's
> an ARPA project or a Service project, and that's
> good for many of these things.... However, it does
> make it a little difficult ... to figure out what's
> going on, because there are no central themes of
> large scale at the present time. There are 40 of
> them, each of them [in the] $5 to $10 million
> class.... But you can't say what central, national
> problem is this collection working on ... that
> isn't there.

New leadership personalities, of course, could attempt to change this
situation overnight. Nothing legal has been done to foreclose re-creation
of ARPA's role in DEFENDER or VELA. Dr. Rechtin, however, believes that
it would be extremely difficult to do:[32]

> If ARPA had tried to keep itself in the national
> spotlight, perhaps there would have been national
> problems that ARPA would have been handed [by now].
> Who knows? I don't....
> I'm not sure it's practical [to have ARPA work
> on national-level problems]. The reason is, how the
> hell are you going to get the Congress to OK it?
> They've already insisted that ARPA's budget be
> broken down in sufficient levels that you can fol-
> low all these projects at sort of the $5-$10
> million class. And if somebody came in tomorrow
> morning ... and said 'we want to aggregate a few
> of these in the interests of better management' ....
> we'd get shot right out of the saddle.*

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* Ironically, ARPA may be involved in a current Presidential issue, namely,
the role and implications of the strategic cruise missile. According to
one report, "the present incarnation" of the cruise missile uses a turbo-
fan engine concept based on one developed by ARPA in the late 1960's for
a "jet belt." This was the AGILE work aimed at using jet propulsion
packages to lift individual soldiers. (Deborah Shapley, "Cruise Missiles:
Air Force, Navy Weapon Poses New Arms Issues," Science, February 7, 1975,
416-418.)

Nonetheless it might be argued that a return to larger-scale, "Presidential issue" problems could be worth the risk. This is probably a debate worth having, a quasi-philosophic thinking through within OSD of what an institution like ARPA might best be used for, over time. We will return to this point below.

"Avoiding Technological Surprise." One mission claimed by some of the ARPA Directors in both the Presidential issues and non-Presidential issues camps is that of guarding against technological surprise. ARPA's "anti-surprise" mission has always been a bit vague, frequently misinterpreted, but often repeated because the rhetoric has a resounding ring, as does the companion phrase frequently used by Dr. Foster, "achieving technological superiority." Roy Johnson clearly started out with the task of investigating outer space technology (including the ballistic missile defense problem) on an accelerated basis in order to guard against other Soviet surprises in that medium. Even the propellant chemistry and materials sciences program assignments were justified and discussed in terms of their relevance to missile and space systems. Furthermore ARPA was consistently described explicitly as a program-oriented or project-oriented agency. The Services zealously monitored ARPA for several years to insure that it did not undertake a whit of work without formal assignment, in part because of the fear that ARPA might acquire a monopoly over important weapons R&D and compromise their respective futures. Thus ARPA was in no position to be a general purpose "anti-surprise" agency. General Betts has made it crystal clear he believed that the Agency had no such mission when he directed it (see Chapter IV). Nor apparently did Dr. York, the DDR&E at that time. Asked if ARPA had caused him any particular disappointments, measured against his expectations for the Agency, Dr. York replied:[33]

> No, not really. I don't think so. Because I didn't really expect any of the problems to actually be solved. In other words, the fact that ARPA didn't come through with any breakthroughs was not a disappointment.

Dr. Sproull, however, was and is a very keen supporter of an ARPA "anti-surprise" mission:[34]

> I regarded that [guarding against technological surprise] as the heart of our mission, as a matter of fact, I didn't regard those 'buzz words' as pejorative at all. They are 'buzz words,' no question about that; they were ... in those days. But the fact is that I regarded it as one, if not the principal mission of the Agency: to make sure that there wasn't something that had been turned down by the Services as too risky, or impossible because of the way the Defense Department is

> divided up into Services, something that fell in
> the cracks, that the Soviets were going to come up
> with, or somebody else.... That ARPA would be the
> people that the Secretary and the President would
> have the right to say 'well the reason I have ARPA
> is just to make sure that we don't get that kind of
> shock.'

But note the assumption of Presidential and Secretarial involvement and concern. Moreover there are additional qualifications crucial to Sproull's conception of ARPA in this role:[35]

> The budgets that we had in those days -- which were
> $300 million, give or take a little -- were a hell
> of a lot of money, if you played your cards right....
> The type of surprise that we are trying to protect
> against was the Soviets building a laser before we
> even knew that a laser existed. And if we had good
> enough contacts with the academic community and if
> we put money out to goose the strong people in the
> Service laboratories and in academic laboratories,
> we couldn't guaranty that, but we could work as hard
> as we knew how on it and we could have had a fair
> chance of success in protecting against [a] Soviet
> success ... that would be a definite change in the
> balance of power, from a scientific invention. We
> took that extremely seriously. It's true that a
> lot of our money was poured into continuing programs
> and wasn't all that flexible. On the other hand,
> we kept some 6.1 money in almost every project,
> some way or another, and we did a lot of bootlegging
> of things in DEFENDER, for example ... you can't put
> your money out to ward off technological surprise
> and not have mistake after mistake after mistake.

ARPA then played an anti-surprise role largely within the confines of very broad program assignments like DEFENDER and within a very large total Agency budget (a third larger than present day budgets in current dollars and much larger than that in constant dollars). The large programs and large budgets permitted 6.1-type "gambling" and also provided sufficient "background noise" to absorb mistakes. 6.1 work in DEFENDER, for instance, was easy to justify as relevant simply by defining it as related to missile defense R&D. ARPA was both confident enough and big enough in dollar terms in 1961-1967 to do high risk projects and not worry unduly if they failed.

Latter-day ARPA's appear to have lost much of that flexibility. Some program titles such as Strategic Technology and Tactical Technology are even broader than Ballistic Missile Defense or Nuclear Test Detection;

however, the assignment within them of bite size program elements, very
discrete projects with reasonably precise relevance or transfer ration-
ales, and relatively low budgets, makes carrying out a true anti-surprise
mission somewhat unlikely. There is no margin for "glorious mistakes"
any longer; they are no longer as readily hidden or absorbed, although
intelligence-related projects sometimes offer scope and justification for
doing so.

Some of our respondents consider discussion of whether ARPA is capable
of playing an anti-surprise role irrelevant because the concept itself is
faulty. Dr. Ruina, for instance, believes it is foolish:[36]

> I never believed in a surprise.... The whole idea
> of surprise is somebody is going to open the door
> and out comes an operational system. It's just
> madness. There's no history that that ever hap-
> pened.... What you're surprised by is, sometimes,
> an experiment that they do.... But the implica-
> tions are that there's a surprise, that a door
> opens up, and there is an operational capability
> that you couldn't have. That never happens. And
> it's just fantasy. Why do they keep this tremen-
> dous air defense structure? We are closing down
> ours; why are they keeping their's alive? And
> it's costing [them] billions a year.... That's
> the nature of surprise. But a technological sur-
> prise changing the strategic balance is just mad-
> ness. You know, I don't think I ever felt that way.

Dr. Rechtin, it will be recalled, sought to follow Foster's instruc-
tions to return to the fundamentals of the early ARPA, which they tended
to define as "anti-surprise" and "quick-reaction:"[37]

> So I went back to the foundations of ARPA, essen-
> tially, and said to avoid technological surprise
> [is] to do the things we weren't sure of what the
> end military mission would be; you just knew that
> there would be a military mission of value. But
> you couldn't tell us where the best value would
> show up. You almost never can. You can't even
> tell that in a weapon system, much less on some-
> thing in research.

As noted, this was extremely hard to do in the context of insisting on
relevance and rapid transfer. Looking back on the experience, Rechtin
adopts a much more Ruina-like posture:[38]

✱   You can't do anything fast enough in this business,
that the other side can't compensate for before it gets
disastrous. In other words you can't come up with
an overwhelming operational advantage in any short
time. The systems are too big to do this. Now,
you can come up with a technological surprise. You
can come up with a Sputnik, or you can come up with
a new ASW technique. But you can't make it effec-
tive for some years. Three years minimum. Ten years
for some of these things. And in that time there's
a chance for a counter move, to keep it from being
a disaster.

Lukasik too, as noted elsewhere, said that given ARPA's circumstances he
found himself believing in the anti-surprise mission less and less: "I
found that that was an idea that didn't wear very well."[39]

✱   Foster, alone among the many people interviewed for this study, re-
mains a strong, essentially unconditional "anti-surprise" enthusiast. He
observes that at the time of Sputnik the U. S. was putting two or three
times as much effort into science and technology as the USSR. Now he
estimates that they are ahead of the U. S. on this measure and that per-
haps 70-80 per cent of Soviet R&D resources is devoted to military capa-
bilities; hence it seems reasonable to him to expect a crisis soon and that
it will be necessary to reinvigorate or reinvent an ARPA: "Today, much
more than 1958, you need an ARPA."[40] While a minority view in our sample,
it is not to be dismissed lightly. A Soviet demonstration of, say, some
strange new orbiting offensive weapon, could result in a reaction not unlike
Sputnik and a modest witchhunt to determine why the U. S. was caught so
unprepared.

Basic Research Role. Few things have consistently polarized feelings
about ARPA, from 1958 to the present, as clearly as the debate over doing
basic and applied research (6.1). Betts, Ruina, Sproull, and Herzfeld for
instance, are very keen on ARPA 6.1. Dr. Killian remains adamant that DOD
has an obligation to support basic research and that an agency like ARPA
is a good one to do it.[41] Ruina today assesses the institutional value
of ARPA largely in terms of its performance in the 6.1 area, in part be-
cause it appears that "high quality research" and 6.1 tend to be synonymous
in his thinking. Asked to assess ARPA's level of accomplishment over its
lifetime, he developed the following portrait:[42]

Compared to what the rest of the Services did, my
guess is they [ARPA] did better. Compared to what you
really can do with that kind of money, I'd probably
given them a C minus. [ARPA] should have been
pushing more promising technologies and better
people all the way." I think the Arecibo thing

could have been done ten times over in different
fields as far as the basic research end.

Rechtin and Foster sought to remove ARPA from what they felt was an over-
concern with 6.1. Lukasik moderated that tendency, but did not reverse
it, seeking to justify some basic research on the ground that it was the
best source of "breakthroughs," but retaining pride of place for 6.2 work
because it would produce more "successes." There are vast conceptual and
semantic problems in this subject beyond the scope of this paper. Suffice
it to say that 6.1 is one man's definition of truly "advanced research,"
but another's idea of chasing rainbows -- both may be equally dedicated to
the search for revolutionary technology and using ARPA to support it.

Oddly enough, as ARPA has aged, the idea has been broached on several
occasions to centralize all DOD 6.1 R&D in the Agency.* ARPA itself has
never been the source of this suggestion and only one of its past Directors
would be inclined to support it. Indeed ARPA has tended to be embarrassed
by such a notion because the Services believed that ARPA must be promoting
it.

General Betts has concluded that the idea has some appeal:[43]

> It could be done.... [T]raditionally it hasn't
> been, [but] I guess if I were reorganizing the
> whole operation right now I'd be just as happy
> to see a 6.1 effort managed centrally. [Pro-
> vided] the Services are represented by people
> who know the Service problems in whatever staff
> of DDR&E managed that central effort.... I
> would do it in ARPA.

The remainder of his colleagues disagree. Herzfeld believes the Services
must be kept in direct contact with scientists on the outside or they will
lose all receptivity to new ideas, i.e., it is important to keep some 6.1
in each Service program.[44] Sproull did not think the idea was sensible.
Foster was not a proponent of expanding ARPA's limited 6.1 program, much
less an advocate of assigning it the dominant 6.1 role in DOD. Like Herz-
feld, he worried that consolidating all 6.1 in one place "would cut the
guts right out of the Services, and polarize and alienate them."[45] He
also felt, as do most of the ARPA Directors, that coupling the results of

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* For instance, at the time Dr. Sproull took over ARPA, the National Acad-
  emy of Sciences (especially its President, Dr. F. Seitz) urged that ARPA
  seek to become the czar of DOD 6.1. The GAO has called for central
  management of DOD 6.1 based on the conclusion that the Services were
  mismanaging such work, and the Laird/Packard Blue Ribbon Panel on DOD
  reorganization made a similar recommendation, designating ARPA for that
  role.

the hundreds of millions of dollars of DOD 6.1 work done annually to the engineers, operators, etc., in the Services was too big a job for one agency. It would require ARPA to devote an immense amount of its effort to managing an empire of largely routine work. "ARPA has to do mostly 6.2, it really does. It should be aware of 6.1, but do 6.2 mostly."[46] Most of the ARPA Directors accept Foster's reasoning for rejecting the idea of doing all DOD 6.1 research. There are still wide differences of opinion over how much it should do.

Lukasik had to deal directly with the Blue Ribbon Panel Report recommendation and opposed it. He explained his position as follows:[47]

> [Congressional committees asked about the desirability of ARPA centralizing control over all 6.1.] It was a typical case of whipsawing because what they wanted me to do was say 'that was a fine idea and ARPA would be a great place,' and I was volunteering for the job.... [But] I would never bite.... [T]he amount of money we are talking about is some $250 million, which was about equal to or slightly larger than the size of ARPA. And you know that you don't move $200 million in the Defense Department or any agency without a great deal of strong [opposition] ... the Services aren't going to lose that kind of change.... So, I knew that I would be dead bureaucratically if I ever did....
>
> Is [central management] a good idea? I do think yes. Should it be in ARPA? You could not drop $200 or so million into ARPA -- double its size -- and keep ARPA the way it is. We would literally 'kill the goose that's laying the golden egg'.... So you would have destroyed ARPA by doubling it, you would have gotten inefficiencies in ARPA. Congress would not have given ARPA the billets to do it any way. The Services would have killed ARPA, if ARPA had tried to make that kind of a grab. [F]urthermore, it would have badly shifted the balance because now instead of having ARPA 20 per cent 6.1, which meant that ARPA was driven by the 6.2er's -- [in other words] was driven by the tactical and strategic technology office, and the VELA office, the guys [who] were kind of 'in the real world' -- now you would have had basically, you know, a $200-$300 million 6.1 with kind of like a $100 million 6.2. It would have become ARPA's NSF. It would have been a great science operation all right. And then you would have had those poor 6.2er's who ... are critical to communicating with the Services, you

would have lost [them] in the noise.... Further-
more, if Congress is going to do it, it's because
they believe there are some inefficiencies in the
Services. Now, the way Congress works, is, if they
see a $250 million pot of money in the Services
and they think it's inefficient and they decide
it's kind of one-third wasted.... They [will] take
the third off for inefficiency off the top and
keep that and pass on to us the $180 [million].
Expect us to do as much with $180 as was being
done with $250 ... and leave us to sort out all the
guys who were sopping up that remaining third of
the money who weren't getting it. So we would have
gotten, you know, political and bureaucratic hassles
up the gazoo and so.... [I]t's an idea that's better
in principle than it is in practice.

There seems to be little or no likelihood that ARPA will be designated
as central manager for DOD 6.1 research. It is not clear just how much of
a 6.1 role it should play in the future. Granted the NSF is more adequately
funded, will it or other agencies create something like the apparent elite
corps of materials sciences university centers, an Arecibo facility, or the
computer sciences advances attributed to ARPA?

Consider the following remark:[48] *Larry Roberts: 4/23/74*

★ ★ ★

*outgrowth
of basic
research.*

— 6.1 funds.

> ARPA is considered throughout the field as
> being the main supporter and perhaps the most im-
> portant force in the course of U. S. and probably
> world history in the computer.... Although the
> rest of the world has probably heard more of the
> ARPANET than [it has about 6.1] computer work,
> [the latter] was clearly one of the main reasons
> why the U. S. became dominant in the computer
> field, because it had these programs at the varicus
> universities that were developing people and con-
> cepts and ideas, and time-sharing and various other
> things. I think it's had a tremendous impact, cer-
> tainly in the early days when Lick was running it
> ... the country would never have grown in the com-
> puter field the way it did if it hadn't been for
> ARPA.

While ARPA stresses the military value of developments like time-sharing
systems, MULTICS, ARPANET, etc., their greatest importance to the country
may lie in non-defense uses. Moreover, ARPA probably would not have had
ARPANET's to crow about if it had not moved into generous support of the
then-risky 6.1 work which made them possible. In the early 1970's, one

is hard pressed to envision ARPA taking that sort of plunge in an entirely
new area. One senses from the myriad explanations given by our respondents
that the immediate reasons why ARPA reduced this role are quite clear, but
that serious post-crisis evaluation of whether DOD and/or ARPA should,
under some set of conditions, resume similar activity has not been attempted.

Deciding How to Use An ARPA

One of the strongest impressions an observer gains from a brief period of concentration on ARPA's history is the remarkably small amount of
time which has been devoted by senior policy-makers, e.g., the Secretary,
Deputy Secretary, DDR&E, and the ARPA Directors, to considering how best
to use this sort of institution. We are not referring to the annual budget
cycle and the inevitable discussion of what is going to be done in the next
6-12 months. Rather the issue concerns determining what the most signifi-
cant DOD problems are and communicating that in such a fashion that it is
relatively clear to an ARPA just what problems it should attempt to apply
its R&D resources against.

Conceivably one might conclude after undertaking such an exercise that
the Department did not need an ARPA, thereby justifying elimination or a
deliberately-chosen program designed to preserve and strengthen an ARPA
capability for future periods when it was anticipated such an agency would
be needed. On the other hand, the conclusion might be a far more direct
articulation of priority defense issues, as the Secretary sees them, to
guide the scientists and technologists in devising an ARPA program. This
assumes, of course, the original ARPA connection to the special concerns
of the Secretary. The Rechtin/Lukasik experience indicates that this spe-
cial connection was severed and in its place ARPA sought to develop problems
for attention on the basis of closer Service, JCS and unified command rela-
tionships. This indeed may be the appropriate ARPA role, but as the period
under review in this study closes, it is not clear from the record whether
this occurred as a matter of conscious choice by senior decision-makers or
by dint of special circumstance and/or default. There seems, however, to
be a strong circumstantial case for special circumstance and default.

Apparently Dr. Foster mounted a campaign to identify important RDT&E
issues meriting treatment by DOD, including ARPA. He used the Ramo Subcom-
mittee of the Defense Science Board, the science advisory committees attached
to the Services and other agencies, former associates in the DOD/AEC labora-
tory system, etc., and tried to "diffuse down" ideas that emerged into DOD.
He also made a crucial statement about the practicality of the procedure
by which problems are identified:[49]

> There is a hell of a lot of structure for this,
> and it works, but it's limited by the number of
> able people and how long they will work, and by
> the willingness of the Secretary of Defense, Deputy
> Secretary of Defense and the DDR&E to work [it].

Our research suggests that too often this mechanism has not "worked."

The lack of serious discussion at high levels of what an ARPA should
do and how to use it applies not only to identification of appropriate DOD
problems, but related policy questions such as the Defense Department's
and ARPA's future role in 6.1; the possible use of ARPA in 6.3 or 6.4, i.e.,
should ARPA's informal entry into work of this type via the Vietnam emer-
gency be formalized under certain conditions; should a Presidential issues
focus be investigated again or firmly rejected, etc. Dr. Herzfeld confirms
the general absence of that sort of policy debate and makes the highly
relevant observation that "every DDR&E [in the period under study] has
inhibited discussion of ARPA's future." It is as if ARPA over the years
was doomed to fulfill the stereotype of the temporary, stand-by, quick-
reaction agency, available to perform "conveniences." The Agency probably
deserves better, bearing in mind that one of the costs of a more explicit
consideration of its long-term role and program could be termination.

Curiously the absence of institutional discussion of ARPA at high
levels has been mirrored in the Agency itself, with the possible exception
of late 1958-1959 when Roy Johnson's Policy and Planning Division developed
the controversial long-range plan for outer space research and development.
Other than that exercise ARPA itself has done remarkably little long-range
thinking or advance planning as to what it should be doing. The phrase
"advance planning" is somewhat inexact: we do not mean day-to-day planning,
planning how to conduct an approved program, annual budget planning, routine
for-form's-sake extrapolation of current program trends for the next five
years, etc. Above all, use of the phrase does not connote erecting some new
bureaucratic obstruction that in all likelihood would serve to stifle the
flow of ideas from program directors, contractors, consultants, etc., or
compromise those elements of the ARPA style most unique to it, e.g., pr -
viding support to controversial ideas emerging from unexpected sources,
often on an accelerated basis. What "advance planning" is intended to
suggest here is an issue touched on by virtually all our key respondents,
namely, thinking beyond immediate program obligations to consider what
ARPA should be preparing to do or cease doing, a couple of years or more
hence.

Roy Johnson's excursion into these waters failed. The billion dollar
annual estimates for a long-range space exploration program, while fair,
were considered absurd at the time. The shock was so severe that both
ARPA and "forward planning" were discredited. Johnson's Policy and Planning
Division tended to concentrate on space policy issues and setting up pro-
grams and offices for the raft of new assignments that were made in late
1959 and 1960-61. The "planning" function withered rapidly and the Policy
and Planning Division disappeared altogether under Ruina.

Dr. Ruina essentially did his own planning, more or less on a one-to-
one basis with the DDR&E. He did not believe adequate planning could be
done by a group of non-scientists nor did he think strong technical people
would devote themselves exclusively to a planning function. The Ruina

solution was to seek ideas from good people, namely, his own and outsiders such as PSAC:[50]

> Get better people. Nothing beats better people, who are also connected with and listen to and have a kind of peer relationship with better people.

Sproull fell squarely in the Ruina mold. He tended to use his program directors as a de facto planning group, notably Rabinowitz, Frosch and Herzfeld. He never felt the need for a separate central planning staff and indeed considered that one would be "redundant, possibly even embarrassing, if they came up with some report I didn't want to do anything with." [51] Herzfeld's "number one job" for Sproull precisely was to look at problems from the perspective of the Secretary. e.g., "what is it that is spoiling [the Secretary's] breakfast this morning?" According to Sproull, Dr. Herzfeld had a "marvelous capacity" for planning things in the context of the Secretary's concerns. Reflecting the ARPA tendency to "internalize" management functions, Herzfeld was ARPA's planning staff and was able to free people from DEFENDER on an ad hoc basis to explore new ideas in a moment's notice. By reaching out to the staff, JASON, visiting contractors, IDA, etc., Sproull felt that ARPA could adequately cover future planning, even in program areas not assigned to ARPA. It was an informal approach and he agrees that it conceivably might overlook some totally different area of technology where ARPA did not have the right people, but, "if so, I don't think a planning staff would have saved it [ARPA] from that."[52]

Lukasik's conception was much like Sproull's. He doubted the wisdom of having a planning group, particularly one that might develop momentum for ideas of its own at odds with his own. Accordingly his Technology Assessments office was kept very low key and used primarily to test and develop his ideas. He also drew on outside sources such as RAND and JASON. Basically, Lukasik, like Ruina and Rechtin, did his own planning.

The vast majority of Directors, then, preferred to institutionalize planning in themselves or in key individuals in the Agency engaged for the most part in managing substantive, ongoing programs. Most of the Directors averaged about two years in office and a very good case can be made that future planning could hardly be expected to be top priority in their eyes. Moreover the early Directors were part of an era during which it was presumed that, normally, assignments would flow to ARPA, from the White House, the Secretary or the DDR&E. The special connection with higher authority was taken for granted. It was noted previously that the Directors also tended to feel reticent, as relative "short-timers," about committing their successors in advance. Betts, Ruina, and Sproull clearly saw themselves as transients, and Herzfeld and Rechtin probably did too. Lukasik alone had a long tenure, but he devoted his energies more to internal restructuring and adjustments in current Agency relationships than to long-term thinking about the future.

Two of the past Directors take exception to the position of the major-
ity, although both handled this issue "on the job" much as the others did.
General Betts believes that ARPA has paid a significant price for its rela-
tive lack of attention to consideration of the future:[53]

> I think that's the fundamental problem that ARPA
> had all along.... It was a continuing problem for
> the Director of ARPA. I don't remember being ter-
> ribly frustrated by that problem when I was there,
> but I was only there a year and I guess I never
> really got to the point where I was worried about
> what the next program was that we [ARPA] should be
> pursuing.

Dr. Herzfeld similarly believes that this issue has been "handled badly"
in ARPA. He, like all ARPA Directors, references the constant time pres-
sure facing ARPA's leadership. A Director has to make a special effort to
push himself and the organization hard, if there is to be any looking ahead.
He also used organizations such RAND and IDA as substitutes, but outsiders,
no matter how able and experienced, tend to lack the feel or touch required
to sense how best to make a particular agency relate properly to the dynam-
ics of the highest level authorities and issues within a large department.
As a practical matter, think pieces prepared by outsiders never proved to be
influential.

Herzfeld believes that ARPA needs a policy and planning-type unit. He
did not have one when he was Director, accepting the rationale that posi-
tions could not be spared. If he were to "do it again," he would restruc-
ture the organization to include such a unit, with three to five people
and a substantial budget, and endow it with influence within the Agency.[54]

Resolution of this question is not a function of this study. The fact
that serious considerations of ARPA's future has so rarely been addressed
(in other than the "survival" crunches that occurred during the late John-
son, early Betts, late Herzfeld, Franken, Rechtin, and early Lukasik periods)
remains one of the most intriguing aspects of the Agency's history.

Evaluating What the Agency Has Done. ARPA planning in the more re-
stricted, conventional sense of considering whether to discontinue, expand
or modify existing programs and projects is an issue that surfaces in vir-
tually every phase of the Agency's lifetime. Indeed the greatest self-
criticism that emerges from ARPA staff at levels below that of the Director
and Deputy Director (but including some of them) is that ARPA has often
been very efficient at spending money, but weak at knowing in detail what
it produced. Reference is made to volumes of unread research reports and
lack of quality control. With a small staff, it is clear that ARPA pro-
fessionals cannot read every paper that their millions of dollars buy. The
ARPA agent is supposed to do so, and may, but agent performance is a mixed
bag.

Dr. Ruina took the position that significant findings always filtered through, were recognized as such and were promptly exploited, whereas "those things that weren't read, we knew weren't too important."[55] But he recalled that there were no formal evaluation procedures and considered this problem inherent in the short tours of Directors. Few Directors ever saw anything that they started through to completion. The same was true of many program managers, at least in the 1960's. There was plenty of opportunity for unevaluated work to slip through.*

This lack of a formal evaluation system puts a premium on having very high quality staff: to do adequate informal or formal evaluations, weed out marginal or poor work and prevent continued funding of those who do such work. ARPA has had some tendency to overlook work of marginal quality and to continue funding the group in question in hopes that it will do better later, or because it is doing well in some other unrelated ARPA program, or because of toleration (sometimes uncritical) of "failure" as a price of doing R&D, or because the program manager is under pressure to reduce his unobligated balance or else lose the money (and the easiest way to do that is to add funds to an existing contract). For all its success in being flexible, locating new talent to support, and transferring programs, ARPA has supported many ideas, individuals and groups for long periods of time. Today's generation of program officers sometimes do not realize that they are supporting people or ideas that date back to the late 1950's or early 1960's. The following view expressed by a program manager in the late 1960's is typical of remarks made by staff who served in almost all of ARPA's phases:[56]

> When I arrived, the [deleted] part of the program
> was already established, and the contracts in it
> had their own sets of relationships, and it's hard
> to turn people around. Dr. [deleted] had a con-
> sumer for some of his material [in another DOD
> office] and he would relate to them and was impos-
> sible to reorient. Hence this part of the program
> was out of control because of the powerful intel-
> lectual personalities involved. While I had a
> tendency to want to cut people off, I realized that

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* Perhaps the outstanding example of ARPA casualness in this regard is a crash effort undertaken in the early 1970's to evaluate all the transferred DEFENDER programs retrospectively, in response to concerns about a Congressional inquiry. The over-$1 billion spent on DEFENDER was categorized and assessed as to percentage of funds "well-spent" on various projects, in a crash two-day period, using a combined internal staff and contractor effort. The evaluation was essentially intuitive, as there was no file of structured program assessments available for use.

> it was easier just to fund new things and leave the
> old alone. I didn't want to make waves because I
> had to go back to the [relevant university/industry
> environment where research of this type was done].
> All of this worked all right during periods of
> adequate funding, but when the pie shrunk the
> established old people got a larger and larger
> share. It is an unfortunate reality that people
> have just continued to be funded until they
> got in trouble. It's a built-in problem.

As these remarks suggest, some contractors simply "outlast" their ARPA
monitors and manage to keep things going in a relatively unstructured way
because of the inevitable uncertainties and critical time lapses caused by
rapid personnel turnover and other bureaucratic pressures.

The Behavioral Sciences program provides an example, by no means unique,
of the linkage between inadequate evaluation procedures and future planning.
One program manager in that office, regarded as innovative by his peers,
felt very handicapped by the lack of an adequate in-house monitoring capa-
bility. His Service agents did a routine job at best, meaning that there
actually was very little feedback on research quality from them. When the
program had been started, long before his arrival, the idea was to estab-
lish centers of excellence or "pockets" of excellence, gave them two to
five years to perform and then: (1) evaluate the significance of the work,
(2) determine whether there were productive potential interrelations, (3)
separate the sheep from the goats, and (4) reformulate or reshape the
program. The office directors who had this focus in mind, however, were
twice or thrice removed by the time such an evaluation should have been
undertaken. Their successors' attention was devoted elsewhere, reflecting
perhaps the tone of the remarks in the quotation above, namely, it is easier
(and more fun) to start new things than to evaluate or undo the old. At the
broader level, there never seemed to be time to do an overall evaluation of
behavioral sciences results, to consider the value of the "centers," etc.,
and reconstruct new directions for the future. Of course, planning for the
Behavioral Sciences program rapidly became subject to serious distortion
from the bitter political battles over defense relevance and the program's
keynote, of necessity, became "what can be saved" and what types of work
are "safe" politically, rather than what might we wish to drop, modify or
expand based on technical results exclusively. The point, however, is that
historically ARPA has been relatively weak at dropping older work and strong
in starting new work, in large part because the office directors give prior-
ity to the latter activity.

Dr. Sproull reacts hotly to criticism of inadequate evaluation. He
believes that there was sufficient evaluation during his tenure, without
over-management. He deeply believed that in the mid-1960's period that
he knew well -- when so many ARPA successes materialized -- ARPA project

and program managers as a rule knew far more about the substance of the
work they were supporting, and about the substance of work in their fields
generally, than their counterparts in any other federal agency. Indeed
it was primarily because of that characteristic that he regarded ARPA as
strong and unique.[57]

> ARPA had that strength ... and I hope to hell it
> had as much of that when I left as it did when I
> arrived. But that's for somebody else to decide.
> Anyway, there is no question but what it was a
> very strong and unusual agency -- the word 'unique'
> is overused -- but I think it truly was unique.

An ARPA could be reduced to an ordinary, stale bureaucratic agency if it
was forced to install detailed evaluation and planning systems. Indeed
some critics have said as much about ARPA in its later years as layers of
paperwork have been added, e.g., by the DOD Planning, Programming and Bud-
geting System (PPBS), increased Congressional demands for budgetary detail,
Mansfield Amendment requirements, and the Rechtin/Lukasik emphasis on re-
cording and justifying relevance, transfer, etc.* Be that as it may, by
the end of the period under review, Dr. Lukasik felt that ARPA had not yet
mastered the art of "concluding" its programs, that is more or less form-
ally evaluating and recording what happened. GAO has also criticized ARPA
for failing to document the transfer of ARPA projects to the Services and
determining their success or failure after transfer exercises, paperwork
which probably would not contribute to a more effective ARPA in the slightest.
But a more rigorous review of the technical output of programs probably
would. To the extent that relevance and transfer, along with smaller bud-
gets, do make it easier to track work and to impose the discipline required
for adequate evaluation, this problem may be on the way to resolution. The
point, however, is that ARPA professionals strongly predisposed to the ARPA
idea are inclined to mention inadequate technical evaluation, leading to
wasteful support of contractors, as one of the most vexing internal ARPA
management deficiencies.

Presenting ARPA's Record

Intensive review of ARPA's lifetime reveals that the Agency has rather
consistently downplayed what it does. Given all the concern about "why ARPA?"

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* We have not addressed the advent of PPBS during the McNamara regime.
Management and technical people alike in ARPA found it to be a worthless
numbers game. At best it was a scheme for rationalizing R&D decisions
after the fact rather than contributing to the decision-making. At worst
it inculcated a habit of lying or faking because normally it was impos-
sible to predict results from research and exploratory development pro-
grams, and how they were going to be used, for five years into the future.
The exercise was regarded as foolish; the added workload on a small agency
like ARPA was consequential.

questioning, it is remarkable that so little has been done to make clear to its critics and potential supporters what it has accomplished. We are not speaking here of crass public relations, the glossy "P.R." put out by information office flaks; indeed, ARPA has no such office. The issue is whether ARPA has given an adequate accounting of itself to those in authority and the record suggests that it has not. Consequently when difficulties arise, e.g., the AGILE problems with the Secretary, Deputy Secretary, Congress, etc., there often is an inadequate background of known successes against which to weigh them.

In the Roy Johnson period, ARPA was hardly a wallflower when it came to publicity and visibility. The Services bitterly resented ARPA getting credit for space projects and they constantly tried to publicize their own involvement. As discussed in the early chapters of this report, ARPA was under Presidential order to insure that the Services did not plaster their logos, literally and figuratively, on the space projects. This non-technical responsibility greatly compounded ARPA's difficulties with the Services. When the Johnson era ended and York sentenced ARPA to what then seemed to be oblivion, this situation changed dramatically: "After Roy, the matter of ARPA claiming credit for anything became a curse word."[58] The "low key" ARPA from a public visibility standpoint became almost an article of faith for the Agency from then on.

As noted previously, ARPA rarely sought to make clear who the sources of its early assignments were. Space, ballistic missile defense, materials research, propellant chemistry, and nuclear test detection all traced back to explicit White House requests or endorsements. ARPA said little or nothing about that legacy, except in the broadest generalizations. Roy Johnson's disdain for the Killian group and faith in his own standing with the Secretary may explain his failure to do so. But long after Johnson left, ARPA consistently failed to document for the succession of Senators, Congressmen, Secretaries and Deputy Secretaries of Defense, Budget Bureau Directors, etc., that the work it was doing was deemed important at the White House, or in some instances, to the Secretary, i.e., that the work had a claim to national importance. The DDR&E's, more or less in line with their low key acknowledgment of ARPA, likewise had relatively little to say about it, what it was doing, or how significant its assignments may have been. They almost never volunteered to respond to "why ARPA?" questions, although logically the "why" of ARPA's existence and the structure of its assigned workload would properly have been handled at that level.*

General Betts recognized that York very much wanted to move ARPA out of the spotlight in order to reduce USAF-NASA-ARPA bickering over space. Betts was low key in temperament by nature and preferred to deal with the Agency's adjustment problems in as sheltered an environment as possible. Ruina and Sproull were not noted for horn-tooting. Ruina considers this

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* This changed for a time when Dr. Foster was in process of recasting ARPA and reasserting DDR&E control over it.

attitude "a failing in every job I've had ... I've played public relations much too low key."[59] In those days ARPA seemed content to keep the DDR&E happy and console itself with the fact that it knew that it was doing a first rate job. There was also a certain feeling that the less Congress and the Services knew, the less friction would result and accordingly ARPA would create fewer troubles for itself and the DDR&E. Its research results, in other words, would speak for themselves. This admirable rationalization resulted in a self-spun protective cocoon based on the belief that ARPA should lie low, do good things, and eventually all the right people would find out about them. It proved to be short-sighted policy. Many outsiders interpreted this outlook simply as undue arrogance and it may actually have reinforced the critics' propensity to find fault.

An excellent example of ARPA hiding its light under a basket is its research related to penetration aids. Many in ARPA consider it a major contribution to the national defense. Even discounting for the initial high classification and limited access nature of the work, ARPA went out of its way to say very little about it. The following exchange is an excellent example of the customary ARPA approach:[60]

> A: I don't think that pen aids officially identified as such was ever more than 20 per cent of the DEFENDER budget. On the other hand, in my own personal view, which I shared with few people at the time, the real raison d'etre for the reentry program was pen aids. Therefore, the raison d'etre for about half of the DEFENDER program was pen aids. Therefore, I saw it mostly as a pen aids exercise at the end. Now, I shared this view with Sproull, with Ruina, with the DDR&E's, and with McNamara. I suggested it in Congressional hearings, but always in a classified session.

> Q: But not [stated] flat out?

> A: Not really flat out. But people who understood, understood immediately. They said, 'yeah, isn't it great we happen to have all the right stuff in place.' I said, 'yes, it's great.' It was a lucky accident. To a large extent, it was a lucky accident. It was turned into -- that accident was worked very hard -- it turned into a very strong policy.

> Q: We have been right, more or less, in inferring that pen aids probably became the dominant justification for continuing PRESS?

A: Absolutely, because while I thought it was very
important to find out the infinite details of the
reentry physics in order to get a laboratory, if
you will, discrimination capability, I had little
confidence ... that that could be fielded very
effectively, for a lot of reasons.... On the other
hand, I was absolutely sure that that also allowed
me to understand what the other fellow would have
to do to counter our attack, and boy that was worth
every penny spent there.

The Presidential interest in this subject, or that of the Secretary and
the DDR&E, and their explicit decision to use ARPA in this area received
scant attention, much less the work itself. It is important to lay such
groundwork. "Presidential issues" do not recur regularly, but if one is
to have an agency available to cope with them there has to be recognition
in the "down times" that the agency has performed well. ARPA did not do
a good job of sustaining that understanding, nor did successive Secretaries
and DDR&E's.

The "low profile" ARPA of the early 1970's carries on this policy.
With direct transfer of projects rated so highly, ARPA also believes that
it must be very careful to minimize the NIH (not invented here) factor
which can inhibit Service acceptance of ARPA results. Self-effacement is
considered essential to achieving that objective. ARPA's gradual addition
of intelligence-related projects reinforces this policy. Indeed ARPA has
consistently indulged a propensity to engage in intelligence-related R&D
from the early work on reconnaissance satellites to the present day. Such
work serves as sort of a surrogate for "Presidential issues" because the
aura of secrecy involved implies significance; often comes with a built-in
user and relevance justification; minimizes review requirements; and pro-
vides an additional rationale for keeping a "low profile." On the other
hand, ARPA has consistently had management problems with such work because
access limitations do impede adequate peer review.

In summary, in its attempt to survive, ARPA has simply made the "low
key" habitual. As Lukasik puts it: "I don't believe, deep down, then and
now, that ARPA should advertise."[61]

Related to the issue of establishing its record, we return to the
observation that ARPA has not stimulated a broad following or clientele
in the scientific community, despite its reputation for flexibility, will-
ingness to support far-out ideas, commitment to long-term funding of basic
research (for a time), and skill in managing advanced research. ARPA has
its loyal adherents in specific fields, e.g., advanced computer science
and technology and substantial segments of the missile defense R&D community,
but somehow these have tended to remain pockets of support rather than
becoming the basis for strong backing across the board. The early feuding

with PSAC and the President's Science Adviser cost the Agency dearly, creating a level of suspicion or disappointment that never was overcome. Ruina helped immensely with his personal rapport with the scientific elite, but no serious institutional linkage between it and ARPA emerged. Sproull maintained the level of civility attained during the Ruina periods. Dr. Ruina believes that ARPA failed to develop strong vocal backing among many of the researchers it supported because the money was filtered through agents (who were there before ARPA and would be afterwards) and because they regarded ARPA program managers, ARPA Directors, and perhaps ARPA itself as transitory. Thus rather than run the risk of offending the established bureaucracy, they tended to avoid testimonials.

ARPA certainly passed the stage of being transitory and it developed a record of not being cavalier in its willingness to support good programs; however, by that time the highly politicized issues that divided DOD and many in the scientific community -- ballistic missile defense systems deployment, the Vietnam War, arms control and disarmament, etc., plus the great debate regarding the role of science and technology in society generally -- were at work, and tended to swamp any attempts at unraveling a factor as specific as ARPA. Suffice it to say that the advanced research agency stimulated by a scientific elite intent on modernizing and educating the military somehow failed to forge or sustain a lasting link with its progenitors of the scope and depth which might have been anticipated.

By and large, our respondents and the written record portray a most inadequate performance in explaining ARPA. This is most unusual for an agency with ARPA's budget levels. The complexity of much of its subject matter undoubtedly complicates explanation. Greater exposure would likewise generate criticism and increase the vulnerability of some programs or projects, although that seems unlikely after the events of 1968 and subsequent years. Indeed those events tend to confirm the necessity of presenting an accurate and balanced story.

It remains to be seen whether the early-1970's ARPA, based on relevance, transfer, small projects, and the low profile will succeed in the sense of communicating satisfactorily with the higher authorities to which it must respond. The Presidential issue assignments no longer exist. The case seems to rest with the appeal of presenting an ARPA geared increasingly to Service and to intelligence-related requirements.

THE READER'S CHOICE

In seeking a final assessment of ARPA over its various periods, the observer is driven back to personal values and to highly subjective judgments concerning the Agency's influence on science and technology and on the military establishment. Virtually every Director of ARPA and every major program manager has been both praised and damned. The flash and melodrama of Roy Johnson's grand battles over space programs were to many

respondents the peak of the Agency's existence. To others, those days were all motion and little substance. Dr. Ruina's emphasis on research quality and broad support for technological advance was to many, especially participants in that period, little short of heroic -- the essence of what has made ARPA unique. To others, the Ruina approach was the height of folly, leading to severe questioning of the legitimacy of the ARPA mission and ultimately to near-cancellation of the Agency; to these observers, Rechtin's vigorous insistence on transfer and Defense relevance and Lukasik's development of a solid base of Service relationships resurrected the Agency and endowed it with a viable role. To some, however, Dr. Herzfeld's proud defense of a muscular, independent, free-wheeling ARPA, sensitive to the major policy issues of the day, represented the last valiant attempt to maintain an Agency that could "make a difference" in the Defense establishment. Yet to still another group the balanced Agency management and spectacular technical project successes of Dr. Sproull's period were the high point of the ARPA story.

On programs, as with Directors, the observer is given a wide range of choice. Was VELA the hidden ingredient essential to the achievement of the limited and threshold test ban agreements, or did its adjustments to the Defense establishment compromise the nation's ability to conclude a truly comprehensive treaty? Was AGILE a "glorious failure," a voice in the wilderness which, if heard, could have moderated the traumas of Southeast Asia; or was AGILE merely a symptom of Defense Department and ARPA confusion, ineptitude and inability to recognize or admit that they could not cope with a problem? Did ARPA's broad institutional support for materials science, information processing technology, seismology, and other fields fundamentally strengthen the nation's scientific infrastructure, or did it simply postpone the evolution of a rational Federal program of support to science? Was DEFENDER at the core of the development of modern strategic offensive and defensive technology, or was it an expensive luxury providing unnecessary confirmation of the obvious and a scientific and technological hobby shop for developments never to be deployed? The list could be continued indefinitely.

What is in the end most unique about ARPA, taking an historical view, is that there is no other technology-oriented agency in Washington to match it in the pretentiousness of its claims, the exposure of its work to serious criticism and the colorfulness of its style.

The maximist view of ARPA credits the Agency with setting the course for the evolution of U. S. space technology, strategic defensive technology, strategic offensive technology, and the future directions of tactical technology and possibly naval warfare. In other words, outspoken ARPA advocates sometimes claim that the Agency has established the key technological parameters for the Defense Department and NASA. In addition, the maximist view gives the Agency credit for establishing the cutting edge of materials science, computer science, radar technology, seismology and geophysics,

laser technology, quantitative political science, and other areas, plus intervening at crucial times in aspects of radio astronomy, energy conversion, propellant chemistry, climatology, atmospheric physics, and many other fields.

The critics, on the other hand, can see expensive monuments to failure or to irrelevance all over the landscape: the giant ILLIAC IV computer, the sprawling LASA array in Montana, the Arecibo dish, many and varied radars (e.g., PINCUSHION), an abandoned SATURN clustered rocket technology, indirectly a non-operational Safeguard BMD installation, and many others. This is not to mention the many abandoned programs, projects and endless study efforts not necessarily resulting in obvious hardware or results, ranging from the counterinsurgency field units, through far-fetched systems and sensitivity studies to aborted university-industry materials "coupling" programs.

As to colorfulness, there is no other agency that could match a repertoire of investigations including a nuclear bomb-propelled rocket, a "mechanical elephant," plans to orbit millions of bee-bees for BMD purposes, man-computer communications via brain waves, laser and charged particle weapons, and a multi-million dollar program sold through a diagram of a Greek temple. And these are not necessarily even the most exotic entries in ARPA's book of memorabilia. This is an Agency which has designed both super-acceleration rockets and balloons tied to a cable. It is also no surprise to find that Evel Knievel's steam-propelled rocket motorcycle was designed by an ARPA alumnus.

In the end, the reader is left to search for his own net assessment of ARPA's ultimate value, sorting amongst the triumphs, failures, disappointments, and the flaky according to his own standards and expectations. For our part, it does not appear possible to measure conclusively ARPA's influence on the Defense establishment and broad areas of science and technology. The far more exciting proposition is to consider what such an institution might properly be charged to do in the future. If there is a case for an ARPA, it must be because those in authority conclude that the nation and the Department of Defense feel the need, paraphrasing Ruina's words, for a place "that's for fun, not to make a living."

CHAPTER X: FOOTNOTES

1.   Discussion with Dr. E. Rechtin, December 7, 1974.

2.   Discussion with Dr. H. F. York, April 4, 1975.

3.   Ibid.

4.   Discussion with Dr. R. L. Sproull, May 29, 1975.

5.   Discussion with Dr. H. F. York, April 4, 1975.

6.   Discussion with Brig. Gen. C. A. Young, Jr., June 11, 1974.   Col. Dent Lay (discussion, June 17, 1975) is very strong on this point.

7.   Discussion with Dr. R. L. Sproull, May 29, 1975.

8.   Discussion with Dr. S. J. Lukasik, May 28-29, 1975.

9.   Discussion with Dr. R. Holbrook, July 10, 1975.

✓10.   Unattributed study interview.

11.   Discussion with Dr. S. J. Lukasik, May 28-29, 1975.

12.   Discussion with Dr. R. L. Sproull, May 29, 1975.

13.   Ibid.

14.   Discussion with Dr. E. Rechtin, July 7, 1975.

15.   Discussion with Dr. J. P. Ruina, June 26, 1975.

16.   Discussion with Dr. J. Foster, Jr., July 9, 1975.

17.   Ibid.

18.   Discussion with Dr. R. A. Frosch, October 31, 1975.

19.   Ibid.

20.   Discussion with Dr. R. L. Sproull, May 29, 1975.

21.   Discussion with Dr. J. Foster, Jr., July 9, 1975.

22.   Discussion with Dr. R. L. Sproull, May 29, 1975.

23.   Discussion with Dr. S. J. Lukasik, May 28-29, 1975.

24. Letter from Dr. J. R. Killian, Jr. to Lee W. Huff, June 4, 1975.

25. Discussion with Dr. J. P. Ruina, June 26, 1975.

26. Ibid.

27. Discussion with Dr. C. M. Herzfeld, September 17, 1975.

28. Ibid.

29. Discussion with Dr. R. L. Sproull, May 29, 1975.

30. Vincent Davis, The Politics of Innovation: Patterns in Navy Cases, Monograph Series in World Affairs, Vol. 4 (3), University of Denver, 1966-67.

31. Discussion with Dr. E. Rechtin, December 7, 1975.

32. Ibid.

33. Discussion with Dr. H. F. York, April 4, 1975.

34. Discussion with Dr. R. L. Sproull, May 29, 1975.

35. Ibid.

36. Discussion with Dr. J. P. Ruina, June 26, 1975.

37. Discussion with Dr. E. Rechtin, December 7, 1974.

38. Discussion with Dr. E. Rechtin, July 7, 1975.

39. Discussion with Dr. S. J. Lukasik, May 28-29, 1975.

40. Discussion with Dr. J. Foster, Jr., July 9, 1975.

41. Discussion with Dr. J. R. Killian, Jr., May 8, 1975.

42. Discussion with Dr. J. P. Ruina, June 26, 1975.

43. Discussion with Lt. Gen. A. W. Betts, April 7, 1975.

44. Discussion with Dr. C. M. Herzfeld, September 17, 1975.

45. Discussion with Dr. J. Foster, Jr., July 9, 1975.

46. Ibid.

47. Discussion with Dr. S. J. Lukasik, May 28-29, 1975.

48. Discussion with Dr. L. Roberts, April 23, 1974.

49. Discussion with Dr. J. Foster, Jr., July 9, 1975.

50. Discussion with Dr. J. P. Ruina, June 26, 1975.

51. Discussion with Dr. R. L. Sproull, May 29, 1975.

52. <u>Ibid.</u>

53. Discussion with Lt. Gen. A. W. Betts, April 7, 1975.

54. Discussion with Dr. C. M. Herzfeld, September 17, 1975.

55. Discussion with Dr. J. P. Ruina, June 27, 1975.

56. Unattributed study interview.

57. Discussion with Dr. R. L. Sproull, May 29, 1975.

58. Discussion with W. H. Godel, June 18, 1975.

59. Discussion with Dr. J. P. Ruina, June 26, 1975.

60. Discussion with Dr. C. M. Herzfeld, May 7, 1975.

61. Discussion with Dr. S. J. Lukasik, May 28-29, 1975.

APPENDIX A

ARPA FUNDING HISTORY

THROUGH FISCAL YEAR 1975

The following funding figures are based
on an internal budget table prepared by the
Defense Advanced Research Projects Agency,
dated November 4, 1974.

ARPA FUNDING HISTORY
(Dollars in Millions)

Fiscal Year	Amount Requested	(NOA) Amount Appropriated	Program Approved by Congress	Congressional Cut	Program at End of Fiscal Year
1958	–	–	–	–	31.5
1959	520.0	520.0	520.0	-0-	485.8
1960	455.0	455.0	455.0	-0-	340.9
1961	215.0	215.0	215.0	-0-	259.6
1962	186.0	186.0	186.0	-0-	250.7
1963	257.0	250.0	250.0	- 7.0	254.8
1964	280.0	274.6	274.6	- 5.4	283.2
1965	283.4	278.1	278.1	- 5.3	275.5
1966	277.0	274.3	274.3	- 2.7	273.9
1967	262.9	255.9[c]	262.9[c]	-0-	278.5
1968	254.1	248.7	248.7	- 5.4	228.3
1969	244.7	233.2	233.2	-11.5	200.4[a]
1970	238.1	212.1	212.1	-26.0	216.0
1971	222.7	209.0	209.0	-13.7	209.0
1972	228.0	206.5[b]	209.8[b]	-18.2	212.4
1973	226.7	199.7	199.7	-27.0	199.7
1974	210.5	194.2	194.2	-16.3	194.3
1975	216.8	202.3	202.3	-14.5	202.3

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Footnotes to Table:

Prior to FY 1963 ARPA was not on a program year basis, and all uncommitted funds as of June 30 were included in following year program totals.

a $38.0 million transferred to Dept. of Army for ABMDA Program.

b In FY 1972 Congress approved a program $3.3 million in excess of NOA. $3.3 million was obtained by reprogramming FY 1971 & prior year funds into FY 1972.

c In FY 1967 Congress approved a program $7.0 million in excess of NOA. The $7.0 million was supposed to be a reimbursement from Dept. of A. F. for Atlas boosters.